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BY

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AN ANALYSIS OF REFLECTIVE THOUGHT

IN replying to Mr. Buermeyer's criticism of my analysis of reflective thought[1] I suffer from somewhat the same embarrassment that affected him in writing it. He was handicapped by the fact that the analysis which he takes as the subject of his criticism was written for pedagogical purposes rather than for strictly logical ends. I am handicapped in replying by the fact that since Mr. Buermeyer states that he accepts the general instrumental position, and since he develops his own views incidentally in a criticism of portions of *How We Think,* I am not always quite sure of what his exact position would be were he writing to express his independent beliefs. In any case I shall ignore the statements of *How We Think,* and attempt to discuss the points made in Mr. Buermeyer's article on their own merits. It hardly seems to me that the original text naturally bears in all points the construction he puts upon it, but that is a minor matter, and if as acute a critic as Mr. Buermeyer misunderstood it, the text is hardly likely to have been unambiguous to others. So I am grateful to him for this opportunity.

The questions raised by Mr. Buermeyer concern the matter of steps or processes of thoughts. Starting from my analysis into (i) the occurrence of a problem, (ii) its specification, (iii) occurrence of a solving suggestion, or supposition, hypothesis, (iv) elaboration of suggestion, or reasoning, (v) experimental testing, he adduces reasons for holding that the reflective act can not be resolved into separate steps, and that, especially as thinking becomes more competent or scientific, the second, third and fourth "tend to fuse into one indissoluble act." Part of Mr. Buermeyer's contention I accept unreservedly, and am chagrined to find that I should have given any impression to the contrary. In speaking of "steps" it is perhaps natural to suppose that something chronological is intended, and from that it is presumably a natural conclusion that the steps are taken in a temporal sequence in the order taken

[1] This JOURNAL, Vol. XVII, pp. 673–681.

above. Nothing of this sort, however, is intended. The analysis is formal, and indicates the logical "movements" involved in an act of critical thought. It is a matter of indifference which comes first. Not even the occurrence of a problem need come absolutely first in time. For a scientific man may reason and experiment for the express purpose of discovering a problem upon which to exercise inquiry. Were I writing at the present time and writing a complete statement, I should certainly emphasize the point that the main distinction between uncritical and critical or scientific thinking is that the latter strives to combine as far as possible into one act the functions of inferring and testing. The attempt represents an ideal or limit which can not be attained except in mathematics. But so far as the endeavor is concerned, I accept not merely Mr. Buermeyer's criticism of temporal separation of steps, but his conclusion regarding the fusion into one process of induction, deduction and experimental testing.

Apparently this statement leaves no outstanding differences between Mr. Buermeyer and myself. However, the case is not so simple as this. Mr. Buermeyer says things which imply that in denying temporal separation he also denies any significant distinction of functions, while I still stand by the indispensable nature for logical analysis of these distinctions. Upon this point, however, the embarrassment to which I referred in the opening sentences comes into play. I am not sure just how far he means to go in the direction of logical as well as of psychological identification. In any case, he makes specific statements which point to obliteration of distinctions in logic between induction and deduction, and those statements will be used by me to clear up what seem to me significant logical distinctions. He refers indeed pretty continually to the functions of induction, deduction and experiment, in the course of showing their mutual involution. But whether this reference marks a recognition that induction, deduction and experiment are *logically* distinct from one another or whether it is a mere concession on Mr. Buermeyer's part to the exigencies of stating my own position for purposes of criticism, I am not sure.

This question is the important one, for it concerns the respective definitions of induction and deduction. My impression is that Mr. Buermeyer accepts the traditional statement of induction and deduction as logically movements respectively from the particular to the general and from the general to the particular. My chief concern is to modify this tradition. Induction I take to be a movement from facts to meaning; deduction a development of meanings, an exhibition of implications, while I hold that the *connection* between fact

and meaning is made only by an *act* in the ordinary physical sense of the word act, that is, by experiment involving movement of the body and change in surrounding conditions. These are the points to which I hold, surrendering to Mr. Buermeyer as unclear and inadequate those portions of *How We Think* calculated to leave any other impression. There are of course points of contact between the traditional statement and that which I have just made. Facts, data, are logically speaking particulars, while meaning functions as a universal. But the traditional discussion takes either particular or universal or both for granted as given, while I am trying to account for them, and to account for them in terms of the reflective transformation of an experienced situation from a confused and uncertain state to a clear and coherent condition. In this process, data with their particularistic function present themselves when the situation is subjected to analytic observation; they represent the attempt to specify the problem. Suggested meanings present themselves as the means of restoring unity, coherence and consistency to the particulars. As such they have the function of universality. Experiment is the indicated application of meanings to the particulars to see what happens—to see whether the suggested unification can be carried out and maintained. Experiment has a two-fold function. From the side of suggested meanings it is a *test;* from the side of the otherwise fragmentary data, it supplies organization, system. There are various things in Mr. Buermeyer's criticism which indicate that he accepts the traditional idea of ready-made or given particulars and universals, data and meanings. At this point, we part company.

The explicit discussion of induction and deduction may conveniently begin with a reference to the fact that the foregoing statements assume but three functions; while the one quoted earlier includes four—leaving out the problem in respect to which there is no difference between us. This seeming discrepancy is due to the fact that the text of *How We Think,* with its practical pedagogic aim, was especially concerned with enforcing the difference between uncritical and critical thinking.[2] Now one of the most marked differences between poor thinking and good thinking is the former's premature acceptance and assertion of suggested meanings. One of the marks of controlled thinking is postponement of such acceptance. Consequently I inserted between the problem and the presentation of a suggestion the requirement of analytic examination of the "facts

[2] The reader should not be misled by the fact that Mr. Buermeyer and I use the word "reasoning" differently. He uses it to express what I call critical or reflective thinking—thinking in its eulogistic sense. I prefer to confine it to "ratiocination" or rational discourse, the elaboration of implications.

of the case." This "step" is not however different in critical thinking from the step of a tentative or hypothetical adoption of a suggestion. It marks the endeavor to *control* the form which a hypothetical meaning takes. The point as regards my view of induction can be made clearer by distinguishing between the inductive movement and induction as a critically executed function. Both terminate logically in an hypothesis. But in ordinary thinking no pains are taken to control the formation of the hypothesis. In critical or scientific inquiry great pains are taken to secure an accurate specification and collection of observed data as the means of control. This "step" is in my opinion the characteristic trait of *scientific* induction. There is no implication that suggestions do not arise until this step is taken. On the contrary suggestions swarm and press for acceptance. That is the very danger against which systematically executed analytic observation protects us.

Mill set out to lay down rules for induction which shall be as stringent as syllogistic rules were for Aristotelian deduction. Now as against this point of view it seems to me essential to maintain that the occurrence to the mind of explanatory "causes" is not a matter which can ever be subjected to stringent rules. The elements of individual capacity and of accident can never be excluded in the inductive operation. Huxley remarks that after reading Darwin and Wallace their theory of the origin of species seemed so obvious that he could only wonder how he and others who had access to the same facts should not have thought of such an obvious explanation. Here is the factor which can not be reduced to rule as Mill set out to reduce it. But on the other hand, a certain degree of regulation of occurrence of hypotheses does obviously occur. How? The reply of *How We Think*, to which I still hold, is that the analytic examination (extensive and intensive) of observed events supplies such control as is available. This process can be reduced to a considerable extent to rules. Mill's "canons" are not what he took them to be, but they are, especially the method of Difference and the Joint Method, statements of the way to conduct observation in order to secure the data which are *most likely* to render the suggestion of meaning (hypothesis, theory, "cause") relevant and fruitful—"most likely," other things being equal. There is no guarantee like that of the Aristotelian syllogism such as Mill aspired to.

Instead of ruling out Mill's contribution to the theory of induction I tried to place it where it belongs. This consideration supplies my answer to Mr. Buermeyer's remark that my account leaves me without the support of Mill's canons, and in general reduces induction to a mere matter of happy guessing. Just because the phase

of happy guessing *can not be eliminated,* specification of the nature of the problem or analytic observation is of transcendent importance in induction. The performance of analytic observation of course involves experiment; it does not precede it. And it is guided by some idea or suggestion in most cases. Nevertheless there is a *logical* difference between experiment as resulting in data which affect the *formation* of a hypothesis and as *affecting* its acceptance. This is true even when the *same* experiment has as matter of fact both effects. Hence I can not accept Mr. Buermeyer's emendation that instead of defining scientific induction as the sum of processes by which formation of explanatory conceptions is *facilitated* (and regulated) it should be defined as those by which their *acceptance* is determined. Logically, we must distinguish the two results, although practically (as has been said) the ideal or limit of scientific method is that the same concrete procedure should effect both of them.

This is not a matter of splitting hairs. Practically it is essential in order that the hypothetic character of an explanatory conception may be adequately apprehended; and that the one thinking may make certain that a proving or testing experiment brings to light *other* facts than those which have led to its formation and (tentative) adoption. One has only to read current literature on spiritualism and interpretation of dreams, proceeding from men who have established scientific reputations in other fields, to note the practical importance of this discrimination. The history of science is full of similar cases—such as the elaboration of Weissmannism.

The theoretical bearing of insistence upon induction as connected with hypothesizing may be seen by examining Mr. Buermeyer's contention that it is "at least equally deductive in character." I do not differ from Mr. Buermeyer in holding that it will be adequately performed in the degree in which it utilizes deduction. But this statement implies that they are *logically* distinct, while Mr. Buermeyer goes to the extent of holding that they are identical. Deduction involves as he points out "the application of knowledge, of ideas already in hand" (p. 675). Now this he claims shows that forming a hypothesis is deductive, since it rests upon prior knowledge. "Only if we already have some information about a problematic situation, some experience of analogous situations, are we able to form a conjecture not wholly random" (p. 675). And again he speaks of the competent thinker as the one who "focuses upon the case in question all the funded results of the agent's past experience, the occurrence of analogies at once subtle and to the point" (p. 675). Without this deductive quality he holds that hypothesizing would be wholly casual and one man's theory likely to be as good as another's.

The necessity of information about the problematic situation is not only admitted, but insisted upon. It expresses precisely the need of regulating the formation of conceptions by analytic observation which has just been dwelt upon. But it is denied unqualifiedly that this information operates logically after the manner of a premise of deductive reasoning. If it did, any two men with the same technical competency would collect the same data and give their elements the same weight. Suggestion, the occurrence of a hypothetical meaning, is in last analysis a brute fact, alogical. It happens or it doesn't; a certain "idea" presents itself or it doesn't; some other conception holds the mind. (That a similar inductive jump actually takes place in all significant deductions, I do not question. But this does not identify their functions, that is respective uses, values, characteristic results.)

In reference to the use of reasoning from information derived from other and analogous cases Mr. Buermeyer seems to me guilty of a fallacy or confusion which I have elsewhere pointed out. (For example, *Democracy and Education,* p. 187.) There is a difference between saying that the suggestion of a hypothesis would not *occur* without prior experience and that it *rests* upon the use of prior experience. The former is a physical statement, the latter is a logical one. In fact men often have a suggestion occur to them without being aware of the prior experience which enabled the suggestion to spring up. Even in the cases when they are aware of an earlier experience which generated the suggestion they do not, if they are wise, place the suggesting experience and the suggested meaning in the relation of premise and conclusion, but only in that of suggester and suggested—"association of ideas" in common parlance. The logical question is whether the two situations *are* analogous. Knowledge exists and is used to suggest a hypothesis. But the question for *present knowledge* is whether the old case or rule is or is not applicable to the new one. Many of our common errors come from assuming that what is known in some cases is also knowledge for the case in hand. This kind of subsumption is the essence of all dogmatism. Deductive reliance upon old knowledge, that is putting the old case and the conception it suggests in the new case in the relation of logical premise and conclusion, is precisely the thing against which the inductive function has to safeguard us. That a trained man can rely upon old knowledge more confidently than an untrained one is a fact. But the logically trained man still makes a distinction between old knowledge as a source of suggestions and as a deductive premise for a conclusion. The analytic inspection upon which I have laid such emphasis as the crucial thing in induction has to ex-

tend not merely to the present problematic situation but to the prior situations from which a would-be rule or conclusion springs up. Only in this way, can we safeguard the acceptance of a suggestion by determining the degree of similiarity which exists between the two cases. Mr. Buermeyer refers to Newton's inference as to gravitation. Well, why not refer also to his inference as to light? It was the same man working with the same instrumentalities. In one case, the supposition of analogy with prior experiences has been confirmed. In the other case, it hasn't been. In short, the *hypothetical* character of inductive inference lies precisely in the supposition of analogy between the present problematic cases and other assured cases. This is the point at issue. There is no paradox in the fact that what is knowledge in one context is hypothesis or even error in another. But it is a fact which precludes that acceptance of prior knowledge as a deductive premise upon which Mr. Buermeyer's argument depends.

Upon the importance in induction of a plurality of competing hypotheses and of the importance of elimination, of exclusion, of all but one, I am glad to find myself wholly at one with Mr. Buermeyer, and also to accept the explanation which he gives (on p. 676). I should also agree with Mr. Moore that the assumption of a plurality of hypotheses as applicable (that is, as worth trying to apply) to the same set of facts is of the essence of skepticism. But I should add two qualifications. Such an assumption is itself involved in the hypotheses; or it is itself a hypothesis, namely, that each one of a number of hypotheses having *prima facie* claims is worth developing and examining. And I should add also that skepticism about the categorical value of an inductive inference is a prerequisite of good thinking. Part of the worth of competing hypotheses and of the method of successive elisions is that it fosters precisely his healthy skepticism.

But I can not see in acceptance of the importance of the method of elimination anything which militates against my analysis. I can see in it several things which seem to go against Mr. Buermeyer's. For example, since all competing hypotheses are equally suggested by the problematic situation in conjunction with prior analogous situations, they are *not* deductive conclusions from identical premises but are suggestions springing up from different sources. In the latter case each may need deductive amplification and be worth experimental testing. An assertion that incompatible hypotheses are deductively grounded in identical premises is, it seems to me, skepticism of the most nihilistic kind; it destroys the very possibility of any valid deduction. Mr. Moore's statement from this point of view is

quite moderate. But it implies no depreciation of plurality of hypotheses and a process of selection among them by a method of elimination. On the contrary. If one hypothesis is good because it starts one train of deductive implications and initiates one set of experiments, several are better because they extend the operation. In any complicated case, it would be practically impossible to arrive at a sound conclusion save as various deductive systems were compared and the results of different experiments used to check one another.

Mr. Buermeyer's criticism applies also to the treatment of deduction as the development of a suggested meaning through tracing its implications, that is, its logical connections with other meanings or conceptions. His discussion of this point is, however, comparatively brief, and mine must also be. He speaks of deduction in modern logic as "a general theory of types of order, or implication" (p. 676). I see nothing in this conception contrary to my point of view. Because ordinary reasoning constantly employs implicatory relationships in order to expand or elaborate a suggested meaning, there is every reason why an abstract theory of implicatory relationships should develop. Many of those who have engaged in the development of this logic would deny their ultimate instrumental or methodological character. But Mr. Buermeyer's general acceptance of the instrumental logic does not suggest any such disposition on his part. He says, however, that while the development of a content of a hypothesis uses these types of order, it can not be identified with them, since it is partial or selective. Precisely. Just as induction employs selected matter-of-fact information in forming a hypothesis, so deduction employs a system of previously formed conceptions or meanings or relations. It has to pick out and adapt for the purpose in hand, which is set by the problem. This selection is inductive because it involves forming an hypothesis regarding their value or applicability for the purpose in hand. Their established position within the general system of types of implication does not guarantee their appropriateness in the given context any more than the assured place of information in one context guarantees its use in a new problematic situation.

Up to this point I but repeat my agreement with Mr. Buermeyer so far as the use of induction in deduction is concerned. It would also be admitted or claimed that deduction occurs for the sake of experimental testing. More than this, there is constant testing *during* the deductive process; mentally conceptions are tried together to see how they fit for the purpose in hand. Otherwise, as Mr. Buermeyer acutely remarks, deduction would become like the calculations

of an adding machine (p. 680).[3] But this dependence upon and use of induction does not, that I can see, affect the statement that the *function* of deduction, or deduction logically viewed, consists in elaboration of a meaning of which has first presented itself in a crude, undeveloped form, and which is accepted not finally but as worth being made the base of development.[4] In fact, it is not easy to tell whether or not Mr. Buermeyer denies this point.

In conclusion, one ultimate point at issue may be said to turn upon the relation of act (or process) and function, or as Mr. Buermeyer points out the relation of the psychological and logical. I concede wholly to Mr. Buermeyer the contention that in the degree in which any job of thinking is well done, experimenting and deduction are *involved* in induction—and so on all the way around. But I deny that this factual involvement means logical identification. This denial seems to Mr. Buermeyer to imply going back upon that belief in the intimate relation of psychology and logic which is the essence of the pragmatic or instrumental logic. I do not think so. One might contend that a *science* of physiology depended upon ability to detect chemical *processes* and trace their workings with respect to all the functions of the organism. Would this cancel the distinction between breathing and circulation, even if it were shown that they not merely depend upon each other but that fundamentally similar chemical reactions were found in both? Function is not a separate process: to suppose that it is, seems to me the error of all abstractionism and absolutism. It is, however, a distinctive matter, for it concerns the results of processes. A man may go from New York to Chicago and from Chicago to New York upon the same tracks, in the same car and with the same engineer. But the purpose, the outcome is different. So is it with induction and deduction. The more we become aware of the identity of psychological process, the more the difference in function becomes significant. To ignore this distinctiveness of function because of the unity of act seems to me to deliver the cause of concrete, psychological thinking into the hands of the enemy—those who assert that all true reasoning is deductive

[3] I take it that Mr. Buermeyer would probably agree with me that the same thing holds of the general theory of types of order or implications which the new school substitutes for the old syllogism. As a matter of fact, they also involve induction.

[4] If Mr. Buermeyer had carried his criticism to the point of stating that conclusions appear in every phase of reflective thinking I should also have agreed with him. It is far from being true (as a chronological interpretation of my analysis would infer) that conclusion is postponed till the problem is solved. We accept or adopt at every point. The difference is in the conditions and purpose of the acceptance, as is suggested above regarding acceptance of a hypothesis.

and that induction is a mere psychological preliminary, important only in the biography of the individual thinker. I wish that Mr. Buermeyer would turn his well-loaded guns upon that camp; and in conclusion I again thank him for the opportunity to make clearer points which in *How We Think* were doubtless left in regrettable obscurity.

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THE NERVOUS SYSTEM, PSYCHOLOGICAL FACT OR FICTION?

SO habituated have we become to the inferior position which psychology occupies among the sciences that we have become accustomed to excuse its deficiencies rather than to understand and correct them. Hence, the tradition continues that psychology deals with vague and futile materials, not natural facts which can be described and referred to valid laws. Indeed, it is only with extreme reserve that one grants psychology a place among the natural sciences at all. Of course it is the psychologist himself who is responsible for this situation for he is only very slowly prying loose the facts of his domain from the metaphysical incrustations in which the centuries have confined them.

The psychologist's handling of the nervous system is an excellent case in point. The nervous system, originally brought into psychology as a means of concretizing and interpreting the diaphanous and fleeting states of mind, has not yet been provided with its proper place as a component factor in a complex psychological act. Instead, it is mainly used as a scheme wherewith to handle the elusive knowings or awarenesses which are still all too prominent in psychological writings. Although the nervous system is made to do heavy duty in psychology, as is manifest from even the slightest examination of psychological literature, it is only in the case of reflexes and similar actions that it serves in any sense as a descriptive factor. In practically all other cases the nervous system is used in psychology merely as an explanatory agent. In the present paper an attempt is made to investigate the neural conceptions prevalent in psychology with the hope that we can thereby suggest what is factual and what fictitious in these conceptions.

Unfortunately, at present it happens to be true that in general, whether psychologists use the nervous system as a descriptive fact as in the study of reflex action, or as an explanatory instrument in other cases, the results so far as psychology is concerned are

equally detrimental. In general, we might say that two distinct and serious disadvantages are thereby sustained. Not only are the highly important nervous functions gravely misinterpreted, with the consequence that the whole psychological act is hopelessly misunderstood, but, what is probably worse, a barrier is immediately set up preventing future progress in our interpretation of psychological phenomena.

Briefly, let us examine each of the two uses of the nervous apparatus in psychology, and first the descriptive use of it. When we describe a reflex or any other act as a neural apparatus or as an effect of a neural operation we give only a partial description of the activity. Either we make the nervous apparatus the whole act to the exclusion of the muscular, glandular and other processes, as well as the stimulating circumstances, or else, when we include the muscular, glandular, and other response factors, we still exclude the stimulating conditions which are no less essential factors in the whole action. Need we say how inaccurate and useless is the description of an act when we omit from it any factor, whether it is muscular, glandular or discriminative? But we might suggest how seriously inadequate must be an account of a response act from which is omitted the specification of its differential character and specific sensitivity to a particular stimulatng object and condition. To omit the recording of the stimulating conditions of reflex and other psychological reactions means to seek exclusively in the reaction phase of the behavior for the mechanism of the event, which in its essence is an interaction of a complete response with a specific stimulus.

That the nervous system should ever have been made into the exclusive materials of a psychological act may be explained by the great influence of histological and experimental findings upon the thinking of psychologists. Truly remarkable, of course, are the coördinating and integrating functions of the nervous apparatus even when considered as purely physiological (mechanical) functions, but just as certain is it that the narration of how allied and antagonistic reflexes operate as mere facts of synaptic coördination gives us a very slight notion of the exact place of the nervous apparatus in a psychological action. We meet here with a paradox, namely, that the over-emphasis of the neural apparatus in psychological descriptions, instead of adding anything to our understanding of the nervous system, rather deprives us of such an understanding, besides inducing us to place a very erroneous interpretation upon the total psychological act. No one can gainsay that experimental work on the nervous system is absolutely indispens-

able for an understanding of psychological reactions, especially of the simpler sort, but to overlook in such experimentation the pragmatic neglect of many other essential factors, means to misconstrue the facts studied. Let us also remember that in all experimental work the necessity to use simple actions in the laboratory procedure results in an emphasis on the neural[1] factors entirely out of proportion to their actual place in psychological behavior in general.

Turning now to the employment of the neural apparatus as an explanatory factor in psychological interpretations, we find that practically always it serves as a means of supporting a theory of behavior not actually derived from the observation of such behavior. In particular the neural mechanisms are used to uphold some sort of mentalism; that is to say, the neural apparatus is seized upon as an appropriate physical counterpart (either parallel, cause, or condition or result) of mental states. Among the conditions presumably explained by the neural apparatus is the manner in which the "psychic," whether conceived as stuff or process, can operate in a factual world. And so the nervous system is taken to be (1) the tangible counterpart of the intangible psychic; or (2) it serves merely to fill in the gaps (subconscious and association theories) between the functioning of mental (awareness) processes; (3) or further, it is made to operate as the complete substitution for consciousness in cases where no awareness is presumed to be present. We will not attempt to rehearse here all of the difficulties attendant upon the confusion of the nervous system with mind, which inevitably results from employing a neural explanation in psychology. Suffice it to say that it is our fundamental conviction that the necessity to look upon the nervous system as an explanatory principle for psychological processes is for the most part owing to a lack of appreciation of the essential fact of psychological phenomena, namely, the interaction of a complex organized specific response with a specific stimulus.

Many are the specific ways in which the nervous system is used in explaining so-called mental facts or awareness, and always, we submit, with hopelessly unsatisfactory results. We take pleasure in availing ourselves of Holt's excellent discussions[2] of the peculiar interpretations of the neural functions in mental activity. In speaking of the relation of automatic or habitual to so-called con-

[1] We doubt much whether an unbiased judgment would lay greater stress upon the neural factor than upon the glandular or muscular phases, although it seems clear that the interrelationship of response with stimulus would suffer in any such comparison.

[2] *The Concept of Consciousness*, 1914, Ch. 15. Also this JOURNAL, 1915, Vol. XII, pp. 365–372, 393–409.

scious activities he says, "One theory, for instance, has it that the cerebral cortex is the 'seat of consciousness,' while habituated unconscious acts are done by the cerebellum and cord. From which it follows that when a motion is first learned (for this appears to be always a conscious process) it is learned by the cerebrum, but thereafter it is performed by the cerebellum and cord (which never learned it). A most plausible conception! And thereafter, since it can be performed either consciously or unconsciously, a double set of nervous mechanisms is maintained in readiness. Or again, there is a view that 'consciousness' is comparable to resistance, or heat, developed at neural cell or synapse. Unconsciousness in a process is attained when the neural path is worn so 'smooth' that no appreciable heat is developed. When, then, an act has once become automatic it can not be performed consciously, unless the organism relearns it in a new set of nerves. This patently violates the facts."[3] Also, Holt has shown[4] in his analysis of the drainage theory of McDougall that sometimes the attempt to use the nervous system as an explanation of awareness results in the theory that when the nervous mechanism functions least, there is a maximum of consciousness.

Nor is the case any better with the action theory of Münsterberg which Holt himself espouses, for there has never been, nor can there ever be established any relationship between the nervous system and any kind of knowing. All such neural theories succeed only in throwing the nervous system out of its perspective in the total reaction. No less has this been the case when the nervous apparatus is considered the basis for the association of ideas, than when the neural mechanism is assumed to be a basis for consciousness in general. Indeed, in Holt's article from which we have quoted, we are inclined to believe the spirit of the discussion is opposed to the conception that a psychological act is primarily a neural act or that the activities involved in psychological action are due to and can be explained by the nervous apparatus involved. Holt's view when stripped of its traditional neural concretions is not far from our hypothesis that psychological behavior consists of the stimulating object or conditions on the one hand, and the action of the person on the other. Indeed, wherever Holt uses an illustration, his argument is definitely in accord with our own. We deem it most unfortunate that the neural tradition is so strong, since it induces such aberration in our vision of psychological facts as to prevent us from describing human behavior as it occurs and interpreting it in factual terms.

[3] *The Freudian Wish*, 1915, p. 190.

[4] *The Concept of Consciousness*, p. 334.

We would urge, therefore, that psychology should be emancipated from physiology, for it is only when psychological behavior is studied as it actually occurs that justice can be done to the nervous functions as well as to all the other factors in psychological phenomena. How troublesome the neuronic theory is may be judged from the fact that even when psychologists consider that they are studying responses to stimuli the neural prejudice influences them to consider all psychological behavior as merely the integration of reflexes. Two fundamental objections to this procedure may be offered. In the first place, reflex acts belong to the permanent behavior equipment of the individual and are not capable of integration;[5] and secondly, to think of all of our behavior as reflexes or combinations of reflexes means to overlook the great variety and complexity of our actual behavior. Especially can such a conception not do justice in any sense to the complex social, esthetic, and moral adaptations to our human surroundings. We wonder if anyone ever seriously considered the nervous system as such to be of any service in distinguishing between two objects, to say nothing of a difference between the alternatives of a moral issue. But it is implied that in simpler cases the nervous system does perform such functions. Thus, the supporters of the neuronic theory necessarily overlook the presence in the behavior equipment of the person of other very important types of acts besides reflexes, as well as all the other specific facts of human adaptation other than neural action. To deny then, that all of our behavior is reflex in form does not mean in any sense to neglect or deny any quality or value of reflexes, but merely not to ascribe to them qualities they do not have, nor attribute to them vague and mystic properties of becoming something else by concretion and aggregation. For it is inevitable when we make reflexes the basis of every reaction that we introduce surreptitiously and *ad hoc* qualities and conditions which really are not there. An excellent example of this (because in this matter psychologists follow the physiologists) is the case of the physiologists who assume that upon the series of physiological facts which they study there is crudely superimposed another series which they call psychic.

The neuronic theory, we submit, stands in the way of psychologists who would develop a concrete science of actual human behavior, for such behavior, it need hardly be argued, is essentially such a comjlex adaptation to conditions that it is unthinkable that a neural theory could be an explanation of it. Moreover, to cling

[5] In the psychological process of integration we assume that acts lose their identification in becoming parts of larger acts.

to such a theory means to make the reflex act the prototype of human action. And just here is where the extreme deficiency of the neural theory appears, for by retaining it as a general means of explanation we overlook its actual function and value as a component in all psychological phenomena.

If, while ourselves rejecting neural explanations of psychological action, we still seek a justification for the belief in such reputed explanations, we can find it in the overpowering impulse to make a rigid and fixed explanation of such utterly important and exceedingly difficult facts as psychological phenomena are. How vain is such a quest is clear from the fact that quite aside from its violation of scientific methodology (namely, to seek the cause of a phenomenon in a part of itself) we maintain, and with perfect safety we believe, that a genuinely critical search will reveal not a single valid principle of explanation which psychology has derived from physiology—although this does not deny, in any sense, that many valuable psychological principles were worked out by physiologists. In order not to be misunderstood at this point let us forthwith distinguish between the useful, nay, necessary employment of the neural factors as descriptive elements of actual reaction systems from the useless and pernicious employment of the neural apparatus as an explanatory process. We propose with all emphasis to distinguish between (1) the description of the exceedingly important part which the synaptic coördination processes as integrative functions play in every reaction system,[6] and (2) the neural structures and functions which are implied to exist beside the psychological response and to explain it. In plainer words, let us distinguish between the facts which the neurologist and nerve physiologist have discovered and verified and the neural theories which the older psychologists have invented to materialize their psychism. Let it not be overlooked that we do not deny that in many cases the psychologists' imaginary neurology is based upon a germ of fact. A case in point is the elaboration of the neurological fact that the impulse meets with greater resistance at the synapse than in a nerve trunk, into the fiction that synaptic resistance is the cause or condition of such complex action as remembering or knowing. Such inventions consist primarily, of course, in the translation of associationistic mechanisms into neural terms.

It is all very well to desire fixed materials of references with which to secure complex phenomena, but as a matter of fact the nervous apparatus can not accomplish any such purpose, and pri-

[6] *Cf.* our discussion of the reaction system in this JOURNAL, 1921, Vol. XVIII, p. 263.

marily, of course, because there does not exist any need to anchor down psychological facts when we consider such facts to be concrete responses to stimuli, as indeed they are. We cheerfully admit, as we have previously observed, that for extreme mentalists or spiritualists the neural mechanisms do serve as stable supports, but who, we might ask, would be willing to accept a type of psychology needing such support?

To test our proposition concerning the negative value of neural mechanisms in the interpretation of behavior we might consider the case of the child learning to keep his finger away from the burning candle. In particular, we might study Holt's neural interpretation, since this is one of the most recently formulated views based upon a mercilessly severe criticism of other neural explanations. After rejecting the Meynert scheme which James has made into a classic, Holt assumes that the child is endowed with two reflexes, one for extension and the other for retraction. Now the explanation consists in positing a greater "openness" and "wearing down" of the second or retractive path so that it will operate in preference to the first. But even if we agree to overlook entirely the absolutely hypothetical character of the "openness" and "wearing down" of paths, must we not assume, if the two reflexes are present, that both pathways are already open and worn down? Yet Holt finds it necessary to explain how the second or retraction path is opened and worn down relatively more. This explanation which he offers is twofold. In the first place, he asserts that the prolonged pain which the child suffers continues the retraction stimulus for a long period, thus causing the path to wear down. And in the second place, he suggests that just as the first five pedestrians across a snow-covered field do more than the next twenty-five toward making a path, so the passage of a first nervous impulse over a path of high resistance wears it down more than the same impulse would wear an already opened tract.[7]

As to the first point, what does Holt mean by pain? Not a mental something, let us hope. For if he does, he not only abjures the necessity for explaining anything, since by admitting mentalities he need merely associate with pain an "idea of retraction" as in the original Meynert scheme, but he also involves himself in the far worse situation that he can never demonstrate the connection between such a mental state and a nervous mechanism, to say nothing at all of how such a mental state can wear down a neural path. On the other hand, if he means a pain reaction, that is to say, a response in which the person discriminates pain, then the pain reaction clearly

[7] *The Freudian Wish*, p. 69.

can be a stimulus to the child to learn to withdraw his hand from the candle flame. But in this case, of course, the learning consists of the acquisition of a complete reaction system and not the wearing down of a path in the nervous system.

And now let us examine the second part, namely, that the retraction path is worn down more because it is a new path. Here again it is difficult to see why the retraction path is new, since Holt assumes the two reflexes to be present. And as to establishing a balance between the two,[8] in what sense is that learning to keep the finger out of the flame when in fact the balance means merely that both acts will be performed. That is to say, each time the child puts his finger into the flame he will also withdraw it. No, Holt must cleave to the notion of a greater openness of the second path, and not merely a balance between the two, but, even if we allow that the retraction path is new and that a new path is opened more at first, how is Holt's problem any nearer a solution? What more can happen with the retraction path than that it reaches the condition of the extension path when the latter first began to be used.

To us the entire explanation is exceedingly fantastic, and for the reason, we might suggest, that Holt is attempting to make the entire learning a neural affair,[9] even to making the stimulating situation (the pain reaction) a factor in the neural process. Were it not for the faith in the neural theory as an explanatory mechanism it is doubtful whether both reactions would have been considered reflexes at all. Now if we are correct in assuming that Holt's handling of the neural theory is as effective as any, then we mean to suggest the possible incapacity of any of them to account for psychological facts.

Very differently is the learning explained on the organismic-response[10] and stimulus basis. As a matter of fact, although it makes little difference for the explanation, we need not consider the first or extension act as a reflex. Be that as it may, we consider the act to be present and because the result is disastrous or unsatisfactory (painful) we observe a new act to be built up. We assume that the withdrawing action constitutes a new response built up for adaptation to the candle as a consequence of the previous reaction to that candle and in addition to the retraction reflex. In other words, there is a new behavior segment established in which the candle constitutes the stimulus. It is the acquisition of this reaction system which constitutes the learning.

[8] *Ibid.*, p. 72.

[9] At this point he is not living up to his promise in the criticism of neural theories.

[10] The term organismic is used to point out the absolute inseparability of the stimulus and response factors in a psychological action.

We assume that in the second or new behavior segment, the child discriminates the stimulus differently or has developed a new meaning for the candle flame, a *new* meaning in the sense that a new differential response is called out by it. This is the essential fact in all perceptual activities. Instead of perceiving the object as something to touch, it is now something that hurts and must be (is) left alone. The essentially perceptual phase of the new behavior segment is a vestigial or incipient performance of the previous reaction, which precedes a final overt response—the withdrawal of the hand or some other mode of action. Especially must we guard here against any implication that the candle flame in the second behavior segment calls out an "idea" of the burn as in the Meynert-James scheme. No such factitious element is in any sense involved in our exposition. The strictly perceptual phase of the behavior segment is an act of the person in precisely the exact form as in the first instance. Moreover, the perceptual act is not in any sense merely a neural mechanism but a complete behavior, although it is true enough that it is not as open to the spectator's observation as the first act. Also we must observe that in the candle-flame situation the perceptual act happens to be a visual response; that is to say, a reaction system in which the primary receptor is ocular, although the complete reaction system does involve in addition tactual factors. The new retraction act, then, is one in which the child's contact with the candle is visual. In fact the importance of the new acquisition lies precisely in the avoidance of any actual touching of the object. But notice, however, that the learning may be just as effective if the new act involves auditory or olfactory perception. And finally, we must be very careful not to confuse the anticipatory perceptual reaction system with the final withdrawal or other response which follows closely upon the operation of the former.

The importance and value of our hypothesis as compared with any neural one, of which we take Holt's to be an especially good example, is further indicated in the fact that it can accomplish two things which Holt admits his theory can not,[11] namely, (1) account for all kinds of learning and (2) explain the child's concept of candle. As to the first, or the explanation of other kinds of learning, from our standpoint all learning, whether manual (handicraft), technical (skill, industrial or esthetic), or informational (book learning) consists of the organization of new behavior segments, that is, specific responses to specific stimulating objects or conditions. Each response constitutes the acquisition of a new specific adaptation to

[11] *The Freudian Wish*, p. 74.

particular surrounding objects. It is thus that capacities and information are acquired by the person.

Concepts, from our standpoint are completely implicit or vestigial responses to surrounding objects. In other words, they are the ordinary perceptual responses so abstracted from the original contact with things through the removal of the stimulating object, that they are aroused to action through a substitution stimulus.[12] Very simply explained, then, are the child's concepts of the candle; they are merely residual responses left over from the original contacts with the candle, and which can be translated into verbal terms. That concepts are derived from originally overt contacts with objects no one will deny, for it is a matter of course that the number and variety of our concepts depend upon our actual past experiences. Also, the degree of abstraction of our concepts depends upon whether our original contacts with the conceived objects were direct (actual) or indirect (imparted to us through speech or printed matter). Once more, unless concepts were implicit actions derived from our actual previous contacts with our stimulating objects, how could it ever be possible to react to these objects in their absence? To repeat, our concepts of objects are the reaction systems developed to those things, which can function relatively independently of them.[13]

Some there are who will still persist in the criticism that after all the organismic hypothesis affords us no intimation as to why reaction systems are built up as responses to stimulation objects. In considering this criticism two points must be carefully distinguished, one of which has no answer. If one means by this criticism that we have not specified why it is that any empirically specific response is developed to a particular stimulus, we might answer that in our argument we assumed that any given stimulating situation would necessarily call out an adaptive reaction correlated with that situation; since, further, we fundamentally assume that psychological reactions are phenomena of adaptation. But observe that the stimulating situations are not the exclusive conditions for the building up of particular reactions. Another very important set of conditions is found in the previous psychological development of the organism, and a by no means negligible circumstance is the biological organization of the individual. Nor do all of these in their aggregate exhaust the conditions for the acquisition of reaction systems; there are many others if only we devote ourselves to a study of psychological phenomena under the factual conditions of their development and occurrence. Most fortunate the day when

[12] *Cf.* Kantor, "An Objective Interpretation of Meanings," *Am. Jour. Psy.*, 1921, Vol. XXXII, pp. 231 sq.

[13] Through substitution of stimuli, as we have said above.

psychologists will give up the ideas that psychological phenomena are simple or that they can be reduced to such partial explanatory terms as are involved in the nervous mechanism. Observe, however, that whatever explanation there be for the acquisition of our particular reactions, it can not exist otherwise than in the study of concrete behavior segments.

On the other hand, if one takes the import of the criticism we have mentioned to be that we do not know how reaction systems are built up at all, we can only question the legitimacy of the question. We take it that we are no more obliged to explain why psychological organisms have their specific properties (for we may assume the fact of building up reaction systems as a quality of the organism) than the physicist is required to explain why bodies fall. Not that we would restrict any speculation based upon fact and the criteria of logic, but we do insist that whatever we believe and assert concerning psychological phenomena must be in accord with observable fact and in harmony with the logic of science. In point of fact, is it not obvious that the criticism just discussed is urged entirely in the interest of a neural explanation, which we of course take to be something different from a psychological description?

Next to the misinterpretation of the entire reaction, by far the greatest damage sustained by psychology from the neuronic theory is the retardation in the understanding of the actual function of the neural factors of reaction systems. If it is true, as we believe, that in much of current psychological work an erroneous use of the nervous apparatus is made, then it appears plausible that we are not acquiring all the information we should concerning the actual operation of that important component of all reactions. Surprisingly little is yet known of the exact workings of the neural mechanisms, and since numerous are the facts to be known it therefore behooves us to let no false hypothesis prevent us from investigating neural mechanisms as actual phases of behavior, that is to say as exceedingly complex coördinating systems, and not as causes of acts or counterparts of invented mentalities.

Not untrue is it to say, then, that the organismic hypothesis is presented in the interest of an emended conception of the relationship between psychology and biology. Instead of considering biological phenomena as merely explanatory schemes for psychology,[14] we must study the physiological facts with which psychology is concerned as actual and essential components of a larger adaptation process, namely, the psychological response. No latitude is

[14] The reader will recall that among the first achievements of "biological psychology" was the redefinition of "consciousness" as a thing or process developed to maintain the life of animals.

allowed us in this matter at all, and we dare not omit any physiological fact, because it is just a fact of nature that all psychological organisms are biological organisms also. This truth, of course, should offer no inducement to the psychologist to use physiological facts or fables to explain the phenomena of his scientific domain. Nor is this evil necessary in any sense when we study psychological responses as definite autonomous events existing in nature. Of course, if we consider the phenomena of psychology to be correlates or adjuncts of physiological facts we must frequently resort to the magical use of the nervous system. But regardless of how easily the words cortical and cerebral roll from the tongue of the psychologist when he wishes to explain some mentalistic fact,[15] the neurologist still can not find in the cortex any of the magical conveniences which the psychologist requires.[16] For example, there has never been any neural machinery discovered to account either for the existence or the association of mentalistic ideas.[17]

Finally, we must not be misled by the overlapping of some of the psychological data with biological facts into distorting such data by the indulgence in general physiological explanations; for in the first place, psychological phenomena are no more physiological than they are physical, and in the second place, the argument that psychology is based on physiology is no more valid than the argument that all sciences, because they are human phenomena, are based upon sociology. The only valid scientific procedure is to accord full recognition to any facts that we study without attempting violently to transform them into something else.

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BOOK REVIEWS

Christianisme et Neo-Platonisme dans la Formation de Saint Augustin. CHARLES BOYER. Paris: Gabriel Beauchesne. 1921.

L'Idée de Vérité dans la Philosophie de Saint Augustin. CHARLES BOYER. Paris: Gabriel Beauchesne. 1921. Pp. 233.

We have bracketed these two treatises, not merely because they

[15] An explanation that frequently takes the form of thinking that specific brain cells are connected in some way with particular thoughts.

[16] In similar fashion when the physiologist hits upon some fact which the mere study of neural mechanism does not and can not explain he utters the magic word "consciousness."

[17] *Cf.* Herrick, *Introduction to Neurology*, 1920, Ch. 20. While we can not accept in the slightest Bergson's metaphysical substitution for the neuronic theory (*cf. Matter and Memory*) we must nevertheless commend his excellent exposure of the defects of that theory.

are the work of the same author, but because they bear upon two distinct phases of a very large theme and serve admirably to deepen our appreciation of one of the most notable and dominating characters in the early development of western European thought. The modern world, or that portion of it, at least, which takes little immediate interest in the successive philosophic positions of Latin Christianity, has drifted a long way from St. Augustine and the speculations that made him so commanding a figure in the course of the centuries even among thinkers who have had no obvious concern with theology. Scholars like Picavet, or Harnack, or Loofs, may call attention to particular phases of his genius, and a group of students here and there may read and note, but, on the whole, interest in his writings has flagged, and he remains for the philosophic world at large, even in centers that are supposed to cultivate a tradition of familiarity with his views, one of that great brotherhood of authors who are more written about than read. Yet he is as actual and compelling in his wistful outlook on the mystery of things as Plato or Aristotle, and more modern, if less didactic, than Thomas of Aquin himself.

M. Boyer, one may be sure, is persuaded of all this; yet his first treatise, in spite of many an apparent statement to the contrary, limits the scope of his essays to two distinct points which he develops with a precision and an economy of argument that is as scientific as it is conciliating and restrained in tone from first to last. In the earlier volume he traces the story of the retreat at Cassisiacum, and maintains, against Boissier, Harnack and Loofs, and notably against Thimmes, to name only the more distinguished scholars with whom he takes issue on this point, that it was to Catholic Christianity, and not to Neo-Platonism, that the mind of Augustine yielded itself in the famous garden scene that took place at Milan in the month of September, 386. The succeeding nine years were years of development; but they were years of orientation, too; for they were the years of a neophyte who felt—perhaps too confidently—that he had discovered in the Neo-Platonic School a rational support for the change in his mental outlook that his "conversion" had inevitably forced upon him. The *Confessions* that all the world knows and the *Dialogues,* a purely philosophical work of absorbing interest and great beauty, in spite of the note of youth that pulses through it, furnish the ground of this contention. They simply will not bear the interpretation that the critics mentioned above have put upon them; and M. Boyer's pages literally bristle with subsidiary fact and text in corroboration of this more reasonable conclusion.

In the subsequent work, *L'Idée de Vérité dans la Philosophie*

de Saint Augustin, published soon after *La Formation,* M. Boyer touches upon matter that is more actual to the present-day student of philosophy. Realist, Conceptualist, Pragmatist, Bergsonian, or Neo-Scholastic, each one of us has his metaphysic—some of us, perhaps, a *meta-metaphysic*—on the meaning of Truth; and not a little depends on the texture of mind with which he approaches the problem.

In the years before the War there was a good deal of discussion on the subject, hardly any of it thorough, or profound, unless we except Mr. H. H. Joachim's really able essay. If the general movement of ideas has since set in other directions, M. Boyer's study will not on that account suffer from a lack of timeliness; for there is something, not merely fundamental, but ineluctable and far-reaching, in the answers that one gives to these questions *in limine.* One's mental states, the quality of one's assents, one's ultimate inferences,—these things are involved in them from the start. All this was felt as acutely in St. Augustine's day as in our own. The prevailing skepticism borrowed a certain tone and *éclat* from the brilliant thinkers against whom the *contra Academicos libri tres* were directed from the villa at Cassisiacum. Truth, after all, was a discoverable thing; and the great neophyte was determined to bring that assurance home to the inquirers of his time. *Reducendi mihi videntur homines . . . in spem reperiendae veritatis,* he wrote in 386. It was an ambition in every way worthy of so tireless and high-souled a thinker.

M. Boyer sketches for us in lucid outline the general drift of the argument by which this life-purpose was carried out. He distinguishes four different senses in which the word *truth* is employed by Augustine throughout his writings. There is, first of all, the common, or *logical,* sense which pervades all the more recondite uses of the term, and which, in a sense, may be said to inspire them. It is the assertion or affirmation, of *that which is,* the identification, namely, of reality with whatever may be affirmed of it. From this logical sense of the term the transition to its more metaphysical suppositions may be inferred. There are three of these: (α) the *adequational* sense, as embodied in the relation between the thing and the idea; (β) the *Logos,* or filiative, sense, known only through the Christian revelation, in virtue of which the Son is the unique and perfect Expression of the Father; and (γ) finally, the *graduate* sense, which is to be found only in finite, or derived Reality, and which is rooted in the measure, or grade, of being that things have as imaging in their several likenesses, or adumbrations, the primary reality which gives them their measure of truth. It is in this sense

that the Scholastics, later on, maintain with Aquinas that *things are true in proportion to their being; maxime vera sunt maxime entia.* That St. Augustine in his various expositions of this aspect of *the truth of things* laid himself open to grave misapprehension in after times, many a chapter in the *Summa contra Gentiles,* to say nothing of the various Schools of Ontologism in the last century, abundantly proves. M. Boyer, though but a modest beginner, is to be congratulated on this really dignified and scholarly piece of work.

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Dieu—Son Existence et sa Nature. F. R. GARRIGOU-LAGRANGE, des Frères Prècheurs. Paris: Gabriel Beauchesne. 1915. Pp. 770.[1]

Let us say frankly at the outset that the delay in noticing this work of the distinguished Thomist and Professor at the *Angelical,* whose name is not as well known to American students of philosophy as it ought to be, is out of all proportion to its merits which are many and satisfying to a marked degree. The author describes his book, in a challenging sub-title, as a *Solution Thomiste des Antinomies Agnostiques.* Whether the class of students to whom we venture to recommend it for consultation, if not for exhaustive reading, will agree with this initial claim will depend, of course, on his previous equipment and his general attitude towards the more fundamental problems of epistemology. The work, in spite of its bulk, is professedly a text-book, with a text-book's inevitable limitations; but it will be found to be a very stimulating and wholesome contribution to many a more pretentious effort notwithstanding that fact. On not a few of the problems that the late Professor William James tried to illuminate in his own inimitable and engaging way, F. Garrigou-Lagrange speaks with compelling attention, and, be it added, with a courage as refreshing as that of the great Harvard teacher himself. As might be expected, the range of subjects touched upon in the course of the work is almost as extensive as the history of western thought itself; but the subjects themselves are pressing, actual and modern,—as the array of foot-notes and long citations, embodied candidly in the text, makes clear. Perhaps the chief merits of the book are its downright attitude towards those Kantian strictures on causality which so obsess the modern student of thought, and its obvious acquaintance with the philosophic literature of our time.

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[1] The book is now in its third edition.

Values, Immediate and Contributory. MAURICE PICARD. The New York University Press. 1920. Pp. vi + 197.

While primarily concerned with maintaining certain theses on specific and technical points of value theory, this little book has a much wider appeal. It is at once too limited in scope and technical in treatment to afford the general introduction the present state of the subject makes so desirable, but it formulates and discusses some of the chief problems which such a book must take into account. Part I. deals acutely and lucidly with questions of analysis, origin and interrelation of values. Part II., perhaps somewhat less successfully, with the normative aspects of the problem. Mr. Picard takes a definite standpoint. He describes himself as a Pragmatist with certain reservations. But this prevents him neither from taking seriously, nor from understanding—up to a point—the philosophy of absolute values which he is called upon to criticize.

Mr. Picard sets himself the task of settling a dispute which has long occupied students of value theory, namely the relation of intrinsic and instrumental, or of immediate and contributory values to each other. "My whole thesis," he says, "assumes that there are two classes and that they are of coördinate rank." Everything with which conscious activity comes into contact is valuable from both the contributory and immediate points of view. But it is only the contributory values that are objective. His position may be described as a polemic against the subordination of instrumental to intrinsic values, and against the supposed objectivity of intrinsic values. Starting with points that are matters of general agreement, namely that all contributory values are objective, some immediate values are subjective, his own conclusion is that all immediate values are subjective (p. 4).

It is with the second part of his polemic that the larger part of the book is concerned. The disproof of immediate objective values in the three spheres of morals, esthetics and logic is necessary, but it becomes of the first importance to disprove the theory that truth is an immediate value. For the general question Windelband's theory of norms is taken, for the more specific, Rickert's theory of truth. Mr. Picard has done a useful service in his conscientious critical study of these two value philosophies, a genuine understanding of which, especially of the latter, is signally lacking in American philosophy. There is nothing distinctively new in this criticism, and his apparent lack of acquaintance with the 1915 edition of Rickert's *Gegenstand der Erkentniss* unfortunately vitiates much of his criticism so far as it applies to that writer. He charges both

philosophers with inconsistency and errors of method and in that charge he is undoubtedly to a degree justified. The element of *psychologismus* in both of them has been pointed out repeatedly. Rickert, he finds, adopts the position that truth and reality depend upon a transcendental *Sollen,* but he assumes the knowledge of certain facts, psychological, in order to prove this dependence. Windelband does the same thing. Yet to show internal inconsistency and error in method does not disprove the truth of the position, and it can not be said that Mr. Picard has disproved it. Indeed, in one place he says (p. 177), "our attack is not so much an attack on the theory that there are objective norms of thinking, feeling, willing, as an attack on the attempt to demonstrate the existence of such norms from psychological data." As to the charge of inconsistency and error of method, Rickert at least escapes it, in my opinion, in the more objective method of his later book. In any case this *psychologismus* is not inherent in the position, as I have elsewhere attempted to show.

On the other hand, Mr. Picard's positive contention that all objective values are contributory and do not depend upon immediate or intrinsic values for their objectivity, leads to some curious consequences. "Strange as it may seem," he writes, "the judgment that a vase is immediately beautiful will be found to be of contributory value" (p. 17). Apparent judgments of objective intrinsic value retain their objectivity by being forced, in pragmatic fashion, into the instrumental mold. This is possible, I think, only by a confusion, inherent in all pragmatic discussions of this question, of instrumental (in the sense of objective relation of means to ends) with contributory (in the sense of adding to the functioning of conscious activity). It is in this latter sense only that all *judgments* of value may be said to be contributory. This confusion of the judgment of value as contributory with the judgment of contributory value leads, of course, to the denial of judgments of immediate value. Mr. Picard thinks that such judgments are only apparent and due merely to the fact that "it is possible to use the cognitive function of conscious activity to express in thought and language facts of immediate value." The "value judgment" is a large question into which we can not go here. It must suffice to recall the main contention of the opponents of his view—namely, that value, as merely felt and not acknowledged in judgment, is not a value in any philosophical sense. It is merely a psychological fact, a part of existence.

In this concluding chapter Mr. Picard leaves the way open for a philosophy or metaphysics of value in a fashion which, while per-

haps not wholly consistent with his Pragmatism, is none the less admirable. In this connection it may perhaps be permitted to take note of the author's reference to the present writer in his preface. The "subjective point of departure," the "psychological position," ascribed to me applies only to an early work specifically designed to introduce the English-speaking public to that phase of value theory. That this scarcely represents my *present* position must be obvious to those who have followed the later developments in this field. Insistence upon my present "objective, non-psychological" point of view has value in this connection only because this development in the present writer's thought is typical of a fairly constant tendency in value theory as a whole. WILBUR M. URBAN.

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JOURNALS AND NEW BOOKS

SCIENTIA. October, 1921. *The Part played by different Countries in the Development of the Science of Radioactivity* (pp. 257–270): ROBERT W. LAWSON (Sheffield).–A history of radioactivity researches, with some closing remarks on the question of national contributions. The major contributions have been British, the most numerous German, then follow France, Austria, and America. *Le problème de la luminosité du ciel nocturne* (pp. 271–278): CHARLES FABRY (Paris).–A curiously interesting paper. A diffused light is responsible for the degree of illumination we find in the night sky. Though this light is only a five-hundred-millionth of the intensity of sunlight, yet were it not for this light, we should be able to perceive with the naked eye stars of the eighth magnitude, or ten times as many stars as we now perceive. The origin of this diffused light, whether from particles in nearby space, from faint auroras in our atmosphere, or from untold billions of individually invisible stars, is still a mystery. *L'association des idées dans les rêves* (pp. 279–296): ERNESTO LUGARO (Turin).–Dreams are neither mere associations of ideas following old channels nor the symbols for hidden desires. In dreams the emotions are stilled. The abundant originality which characterizes the sensory picture-show of the dream world has never been sufficiently emphasized. Though there is almost total incoherence to the succession in time, a dream taken at a given moment may present sensorially a wonderfully rich and coherent picture. *Buts et résultats coloniaux de la guerre mondiale. I. Les résultats politico-territoriaux* (pp. 279–308): GENNARO MONDAINI (Rome).–This first article is little more than a compendium of facts concerning the colonial growth of Eng-

land, France, and Japan due to the war. *Reviews of Scientific Books and Periodicals.*

Brandt, Lilian. How Much Shall I Give? New York: The Frontier Press. 1921. Pp. xi + 153.

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NOTES AND NEWS

PROFESSOR JAMES JOHNSTONE read a paper on "The Limitations of the Knowledge of Nature" at a recent meeting of the Aristotelian Society. He held that a candid and impartial survey of the speculative biology of the late nineteenth and twentieth centuries must force one to the recognition of a two-fold passage of nature. According to the fundamental concept of physical science, the second law of energetics, *viz.*, the augmentation of entropy, physical change tends continually to diminution. The universe, to use Bergson's term, is detending. To the biologist there is another aspect, for life is the incessant attempt of certain physico-chemical systems to resist the increase of entropy. The difficulty in accepting the main result of generalized relativity in biology is that for speculative physiology space-time can not be completely isotropie. More especially is this so if with Bergson we regard the quality of duration as the cumulative continuity of life. It is a passage as well as the persistence of that which has passed. The conclusion of the paper was that we must regard Newton's "ocean of truth" as amorphous in structure. The relations that are to be discovered in it are only in it in the sense that they come into existence with the thought that makes the relation.

Dr. H. H. Bawden, of San Ysidro, San Diego Co., California, informs us that he is disposing of his library, and will be glad to correspond with those who may be interested. Among his books he has an unbroken file of the JOURNAL OF PHILOSOPHY, which he values at $90.

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Monthly (9 numbers). Managing Editor, J. Carleton Bell.
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